

THE

## CHICAGO MEDICAL JOURNAL.

Vol. XXV.—DECEMBER 1, 1868.—No. 23.

### THE VIRULENCE AND SPECIFICITY OF TUBERCULOSIS.

*Read before the Academy of Sciences (Paris) by Dr. J. A. VILLEMIN, Professor of the School of Val de Grâce.*

TRANSLATED EXPRESSLY FOR THIS JOURNAL, BY WALTER HAY, M.D., ASSOCIATE EDITOR.

*(Continued from page 729.)*

THUS, then, far from denying any significance to experimental facts long since established, and refusing to perceive analogies the most striking, it must be conceded to us that our inoculations of tubercle have been made under conditions entirely similar to those which have accompanied most experiments of similar character; and, even, in comparison to those of glanders, the corresponding circumstances are multiplied to the degree of constituting an association of phenomena almost identical. Here are undeniable facts—such that no contradiction, however urgent it may be, can refute. It is next asked, where is the serum, regarded as necessary to the constitution of a virus, which should be in the granulations of glanders, the tumor of farcy, the froth of the solipeds, and which is not found in tubercle, nor in the expectoration of phthisical subjects. Moreover, what is known

of the physical condition of contagium? Is it solid, liquid, or gaseous? Do not the brilliant and judicious experiments of M. Chauveau demonstrate to us, on the contrary, that certain virulent fluids have no activity unless they contain solid corpuscles?

In spite of all the efforts attempted to deny to tubercle the properties of virulent substances, no one has, however, been able to refuse recognition to the remarkable power, which its inoculation exercises, of involving the formation, throughout the economy of tubercular productions in great number, and disseminated through the most distant organs. Now, what is comparable to this phenomenon, unless it be the inoculation of virus? However, an analogy as evident has been disputed, and we are reproached with having created at once two difficulties: "the first of which is to establish the existence of the virulent principle; the second to explain how it engenders tuberculous matter." We believe that there are no other proofs of the virulence of a pathological product than its inoculability; and, as to explaining how tuberculous virus engenders tubercle, the difficulty is neither more nor less great than to say how the virus of glanders engenders the tubercle of glanders, or syphilitic virus, syphilitic gummata. We have already stated elsewhere, that the relations between any phenomenon whatsoever and its cause establish, but do not explain themselves.

It is by virtue of a *catalytic* action, analogous to that of ferments, say some, that inoculated tubercle infects the economy with tubercle. Well, has not virus been compared to ferment, and does not the name *Zymotic*, given to virulent maladies, confirm this analogy?

Next, it is asserted that inoculated tuberculous matter acts neither in the manner of a virus, nor by catalysis; it is by a process analogous to that of fecundation. Doubtless it might be asked if, at the termination of the process of fecundation, the fecundating principle reproduces and multiplies itself—if sperm or pollen are recovered from the subject fecundated; but it does not imply the identity of all the terms. Moreover,

if there is in this case a sort of fecundation, it exists likewise in the case of all other virulent substances.

By inoculation the detritus of a chancre gives chancre, the variolous pustule studs the skin with variolous pustules, the tubercle of glanders disseminates glanders tubercles throughout the organs, just as the phthisical tubercle infects the system with phthisical tubercles.

This hypothesis of fecundation is but one mode of apprehending and explaining the action of virus. It is no less ingenious than all the rest which have been proposed hitherto, and, as we shall hereafter see, tuberculosis is not the only disease to which it has been applied. But in the present state of science, I do not perceive any advantage in substituting for the word inoculation, that of fecundation, applied to this fact of the transmission of a disease from one individual to another by means of a particle of morbid matter. If, instead of seeking our comparisons in unexplained chemical processes and in very remote physiological acts, we form them in the ranks of similar facts — if we observe, for example, what occurs in inoculations of glanders, a disease so similar to tuberculosis, what do we see? We determine that all the particulars observed in the inoculation of tubercles are recognizable in the inoculations of glanders.

These are, by their physical characters, the same inoculated matters, the same alterations of the ganglia and of the lymphatic vessels, the same anatomical processes generalized in the viscera, with preference for the respiratory organs. If, when there is introduced into a wound some of the liquid of the froth, or a little of the caseous matter of a tubercle of glanders, or the detritus of a farcy-button, there is produced at the end of a few days, at the point of inoculation, a little tumor which frequently ulcerates; then from this tumefaction there arises a cord, which extends to the hypertrophied hard and painful ganglia.\* Usually, if the inoculation has been performed on one side only, the corresponding ganglia are alone diseased.

\*Bouley, *Bulletin de l'Academie de Medicine*, 1838-39, p. 793.

These ganglia incised appear full of glanders tubercle, and the lymphatic vessels reach their parietes infiltrated with the same product. There is a series of lesions which is reproduced, in a manner exactly parallel, in inoculations of phthisis. Coincidently with these local alterations there are established in the lungs, the respiratory mucous membrane, the liver, the testicles, the intestines, etc., nodules of variable size and number.\* Are there in pathology two processes which have between themselves more of striking analogies than these results of the inoculation of glanders and of tubercle ?

In syphilis, is there not also something comparable ? The ganglionic plexus manifests to us the part performed by the lymphatic system in immediate proximity to the virulent matter.

Thus the objections which have been urged, in regard to the inoculated substance, to the local accidents of inoculation, to the mode of generalization, to the anatomical lesion, etc., apply as well to glanders as to tuberculosis ; and, curious coincidence, these same objections were, in fact, made at the time when the inoculability, the virulence and the specificity of glanders was discussed, just as now. When we established in a work, published recently, a parallel between tuberculosis and farcy-glanders, and when we exhibited these numerous analogies which exist between these two affections, we certainly did not doubt that these analogies would be completed and continued by the similarity of the arguments invoked against the virulence of both of these diseases.†

But before proceeding farther, we must be permitted to correct an error which has insinuated itself into some minds. We are credited with the opinion that glanders and tubercle are identical. We have never thought so. The chapter of our book, in which we have established the approximations of these two affections, has for its title : *Glanders is the disease approximating nearest to Tuberculosis.* We have here devel-

\* Saint-Cyr., *loc. cit.*, p. 65.

† Villemain, *Etudes sur la tuberculose.* Paris, 1868, p. 431.

oped the affinities which exist between these two morbid entities, as might be done, I think, between scarlatina and rubeola, whilst maintaining at the same time the complete and essential separation between the two elements of the comparison. It must not be imagined that the inoculation and the virulence of farcy-glanders was admitted without opposition. The experiment created as great an excitement as did that of tuberculosis. It was vigorously contested in the consequences and even denied radically. Veterinary physicians were for a long time divided into two opposing factions: the contagionists and the non-contagionists. When Gohier, in 1813, had announced the results of his experiments of inoculation, the non-contagionists showed themselves fertile in expedients. At first they denied the fact, and opposed their own negative results to the positive results of their adversaries. They even denied the possibility of the fact. "We ask next, said Dupuy, how *solid matter*, such as that which constitutes the tubercle (of glanders) could become contagious."\* It has been more than fifty years since this argument was adduced against glanders, and now we behold it resuscitated against phthisis. It was especially against the chronic form of glanders which approximates most closely to phthisis, that attacks were directed. And what reasons were made available against its contagiousness and its virulence? The same, *identically the same*, as those which have been adduced against the virulence of tuberculosis. Hear Delafond:† "Among the numerous diseases, of chronic type, of our domestic animals, do we discover a single one which is evidently contagious? We know not one. Now, why should glanders be made an exception? We ask, is it possible to find, amongst all the characteristics of this disease, a single one which may be compared with those, so very numerous, which mark diseases positively contagious? No.

All contagious diseases are of acute, or subacute type; the

\* Dupuy, *loc. cit.*, p. 455.

† Delafond. *Traité sur la police sanitaire des animaux domestiques*. Paris, 1839, p. 603.

symptoms which designate them are constant, unequivocal, their progress is rapid, their duration brief, their termination, although often variable, is generally unfavorable; all have a known palpable *virus*, transmitting the disease by inoculation. Now it is exactly the opposite characteristics which appertain to glanders."

By the partizans of these theories, glanders, like the phthisis of our adversaries, is attributable to no other causes than diminished action, resulting "from long sustained and very exhausting fatigue....from alimentation maintained for a long time with a diet invariable or innutritious....from residence in cold, damp, badly ventilated and badly lighted localities...."from arrest of perspiration"....from long continued suffering from the presence of chronic diseases, whether internal or external, from resorptions of morbid matters of whatsoever kind, which occur during the course of many diseases.\*

Contagion from the horse to man seems not to be sufficiently convincing. Who does not remember the brilliant contest of 1836? And two years later, Delafond refuting Rayer, exclaims again: "No, the cause of the disease called farcy *is not specific*. We recognize in it only the result of an infection, originating in animal matter, fixed or volatile, altered by the presence of air, which, introduced into the economy by absorption, determine morbid effects so much the more intense as the subjects are more debilitated and already predisposed to putrid infection.\*

But when crushed by the evidence, the noncontagionists could no longer resist the power of facts, they change their batteries. "Of what significance," said they, "is the inoculation of the product of glanders, and what becomes of its virulence and specificity, since other substances communicate glanders as well as this — since this disease can even be provoked by simple wounds?"

Dupuy, separating chronic from acute glanders, which he

\* Delafond, *loc. cit.*, p. 595.

\* *Id.*, *loc. cit.*, p. 684.

calls *gangrenous coryza*, induces this latter upon healthy horses by inserting under their skins a *fragment of spleen* taken from a horse dead in consequence of section of the pneumogastric nerve;\* animal substances undergoing putrefaction, such as blood, portions of muscles, etc., will produce the same effect, according to him.†

He induced glanders also in horses by inoculating them with the puriform matter which flowed from the nostrils of a horse not affected with glanders.‡

Renault subjected to the examination of the Academy of Medicine pathological preparations attesting the production of glanders by injections of pus, not even suspected,|| and in order to demonstrate the reality of this glanders, he inoculated it successfully into healthy horses.§ He published detailed observations of farcy-glanders, originating as a sequel to a simple *sore throat*, from a *contusion* of the upper eyelid, from a *fistule* of the spermatic cord, consecutive to castration.\*a

Finally, Dupuy demonstrated that *setons* passed through the shoulders of horses had induced in them glanders.†b

In Germany, Erdt developed glanders in four horses by inoculating them with the pathological products of scrofula (1834). Are not all these experiments, all these assertions, all these arguments, directed against the virulence and the specificity of glanders with as much authority and force as those which are opposed to tuberculosis. If then it is desired to maintain them in order to prevent the admission of tuberculosis into the list of virulent maladies, farcy-glanders must also necessarily be eradicated therefrom, being retained therein with no more propriety than the latter.

\* Dupuy. *Bull. de l'Academie de Medicine*, 1836, p. 481.

† Dupuy, *de l'affection tuberculeuse*. Paris, 1817, p. 244.

‡ Ibid, p. 454.

|| Renault. *Bulletin de l'Academie de Médecine*, 1839, p. 69. 1840, p. 402.

§ Renault. *Recueil de Médecine Vétérinaire*, 1840, p. 257.

\*a Renault. *Recueil de Medicine Veterinaire*, 1835. p. 393.

†b Dupuy. *Bulletin de l'Academie de Medicine*, 1836. p. 481.

The objections urged (I will not say to the inoculation of tubercle, for that is undeniable, but to its virulence and specificity,) have been made not only to glanders, its congener, but to syphilis, its relation one degree farther removed. Syphilitic virus has had, like that of glanders and that of tuberculosis, its violent enemies, and the arms of which they made use are no other than those which have been gathered up again by the adversaries of the specificity of tuberculosis. And, moreover, the first argument was to deny its inoculability and to oppose, to positive experimental results, those which were negative and contradictory. This is what was done by Bru, and in order to account for venereal accidents, he admitted, *un mode venerien*, explained physically by electricity.\* Caron, following the footsteps of Bru, assimilated the transmission of syphilitic accidents to the *impregnation* of females; he explained this by the *fecundation* and not by the absorption and the multiplication of a virus. "It is not," said he, "a virus which is inoculated in the venereal contagion, but it is an occult vice which is developed in us, it is nature, it is life, which establishes the syphilitic constitution." The venereal infection has at first only a local action, which *extends itself successively to certain parts.*† This theory of fecundation is already old, as we perceive, and with it we recognize at the same time the mode of progression, step by step, by which it is attempted to explain the development of tubercles in the organs. Jourdain likewise denied the existence of syphilitic virus, by denying the specificity of the venereal disease, and all significance to its inoculation. He affirmed that "similar and even more grave results are seen to result from a simple puncture."‡ The constitutional manifestations of syphilis depended, according to Jourdain, upon a great number of different causes. Richon-des-Brus,

\* Bru. *Nouvelle methode de traiter les maladies veneriennes par les gateaux mercuriels.* Paris, 1789.

† Caron. *Nouvelle doctrine des maladies veneriennes.* Paris, 1811.

‡ Jourdain. *Waite complet des maladies veneriennes.* Paris, 1816.

in his book, "The non existence of the venereal virus,"\* and Desruelles continued the war against the virulence and specificity of syphilis.

The first admitted the spontaneity of this malady, and the second sought its causes in the seasons, the temperature, hygrometry, etc.

Thus, then, there was a time when glanders and syphilis, like the tuberculosis of our opponents, originated from every thing — was inoculated by every thing.

What is to be learned from these teachings of history? It is that the facts which have suggested such analogous arguments and contradictions must necessarily have among themselves very great analogies of character. In fact, syphilis, glanders and tuberculosis form a nosological group whose species have among themselves incontestable affinities.

If tuberculosis is specific and virulent, it is for that very reason contagious. But it remains to determine the mode and the conditions of its transmissibility. That phthisis may be communicable by inoculation like syphilis, I believe to have been placed experimentally beyond any doubt. It is inoculable from man to certain animals, and from these animals to others of the same species. Is it from man to man? It is absolutely forbidden to apply the experimental test. But every thing indicates the affirmative. To say that it is not so because no one has seen an instance of it, is to refuse to the inoculability of tubercle the right to appear in its day and hour like every other scientific truth. A discovery consists exactly in the summing up of the evidence of a fact which had up to that time eluded observation. At the first report of human glanders, for example, it might have been objected that the thing had never been seen, who then had observed the relations of the phlegmasiae of cardiac serous membranes with articular rheumatism before the beautiful discovery of the *law of coincidences*.

On the other hand the transmissibility of tuberculosis by the act of cohabitation, although regarded as very probable by

\* Richon-des-Brus. *De la non existence du virus venereen.* Paris, 1826.

a certain number of distinguished practitioners, is not, however, so evident that it may not be contested. There is in this matter an obscurity which imposes the greatest reserve, and consequently the clinical evidence, especially in a large hospital, is not sufficient to justify the absolute denial of this mode of transmission of phthisis. Large cities, vast establishments where so many maladies are accumulated, and which are impressed with so many morbid matters, are far from being favorable for the especially difficult study of contagion. In these localities, for instance, the transmission of typhoid fever is almost always concealed, whilst it becomes apparent in small villages, in the bosom of families nearly isolated, to the observer placed in circumstances less complex, more clear, more exact, and on that account more conclusive. And then, moreover, what is the communicable disease whose transmissibility has not been determined? May we not then believe the clinical observer authorized to affirm every where and always the spontaneity of tuberculosis and to deny absolutely the possibility of its transmission by contamination? But would it be right, on this account, that he should infer nothing against the inoculability of tuberculosis? It would, therefore, be much more in accordance with modesty that the solution of questions relative to the propagation of tuberculosis in the human species should be reserved for the future, which will decide, we may be sure, not upon argument and theories, but upon positive and perfectly demonstrated facts. For myself, aided by experiment, I have sought to determine the circumstances which appeared to me to play a preponderating part in the transmission of phthisis.

The curious and important results which I have already obtained, seem to me to be destined to throw some light upon this point. If the Academy will grant me the permission, I shall have the honor to communicate them to it hereafter.

I conclude, gentlemen, by thanking the Academy for the courtesy with which it has condescended to hear the explanations which I have just submitted to it; I am aware, however, that I owe it entirely to the importance of the subject which I have treated.

## DYSTOCIA, ORIGINATING IN A RARE POSITION OF THE FŒTUS.

BY H. WEBSTER JONES, ACCOUCHEUR TO COOK COUNTY HOSPITAL.

THE patient, Bridget D—, was a primipara, æt. 26, of robust constitution, and having arrived at term, was seized with active pains, at 7 P. M., October 18th, 1868.

The abdomen was unusually large; the os uteri high in the pelvis, and undilated, and no part of the fœtus was tangible per vaginam.

Through the abdominal walls the head could be felt, resting upon the right side of the pelvic brim. External manipulation, between pains, succeeded in effecting only its temporary dislodgement.

During the night, pains were frequent and severe, attended by vomiting and restless apprehension.

At 4 A. M., October 19th, the os uteri was found to be fully dilated, with the membranes protruding; the head was discoverable per vaginam, located as before mentioned, and resisting all effort to engage it in the superior strait.

Pulse 110; tongue dry and parched.

At 5 A. M. the patient was anæsthetized, the membranes ruptured (there was an excessive discharge of liquor amnii), and the hand fully introduced within the uterine mouth.

The occiput was found to occupy the right sacro-iliac fossa, the forehead resting upon the pubis, at the right of the symphysis; the head was strongly extended, the back arched upward, and to the extreme left was a foot (apparently the right), with the heel directed inward. Between head and foot was a wide space, in which the hand could be freely moved, occupied solely by a pulsating loop of the umbilical cord.

Efforts at flexing and rotatting the head proved futile; neither

was it possible to draw down the foot to any considerable extent. Dr. Jones was sent for at 8 A. M.

Thus far our able house-physician, Dr. N. Senn, narrates the case.

Upon my arrival, at noon, the patient was found to be in an irritable condition, her pains severe; pulse one hundred and twenty; tongue dry; otherwise apparently in *statu quo*.

Chloroform being now administered by Dr. Senn, I proceeded to verify the diagnosis already made, and, with additional assistance from Drs. D. S. Root and Miller, to deliver the patient by such method as might be most promising.



The accompanying diagram illustrates very nearly the position then occupied by the foetus. Agreeing in essentials with the statements before made, the degree of extension of the head present, taken coincidentally, with the posterior relations of the vertex secured, to favor podalic version rather than a forceps delivery.

Both feet were now firmly grasped, and an effort made, aided by external manipulation, to rotate the body on its transverse

axis, in elevating the head and depressing the pelvic extremity, as well as, longitudinally, to turn the back of the child toward the front of the mother. This complex attempt was successful only in the substitution of an anterior for a posterior rotating of the trunk.

The descent of the foetus upon the latter procedure, was as represented in figure second.



Rotation to the right being still continued, the left shoulder was first to appear at the vulva, the arms having already been brought down.

A difficulty here occurred, in the failure of the head to follow the line of rotation indicated. It was arrested, as shown in figure third,

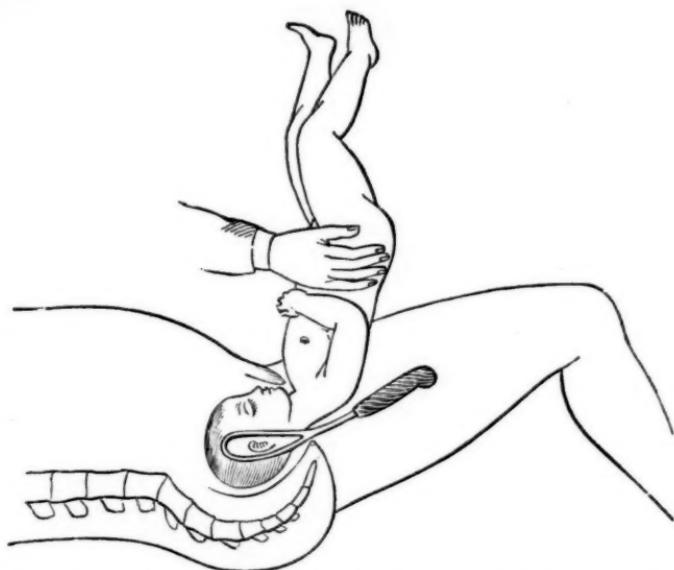


the face looking toward the right groin, with so much of flexion, as that the chin was not caught upon the pelvic brim, yet apparently immovable as regards traction by the trunk, by the fingers within the mouth, or by external pressure above the pubis.

The foetal pulse now flagging, immediate resort to the forceps was determined upon.

It was impossible to adjust the female (right) blade upon the anterior aspect of the face, so that rotation into the sacral hollow could be effected.

Delivery of the head was partly accomplished, as portrayed in figure fourth, the mental extremity of its long diameter finding easy exit beneath the pubic arch.



Life was extinct, the operation having occupied three quarters of an hour, and the cord subjected, meantime, to a pressure almost constantly hazardous.

The mother recovered without untoward symptoms.

The position of the child, as described in the early stage of labor, could have been possible only in the presence of a large excess of amniotic fluid.

The sudden and severe onset of uterine contractions, probably conduced to its maintenance, their immediate effect upon the foetal arch being to separate feet and head more widely, and deter either extremity from the superior strait.

Such a condition is certainly very rare, and is believed to be hitherto unrecorded.

## OPIUM AND STRAMONIUM.

BY JAMES T. NEWMAN, M.D., CHICAGO.

It has long been a subject of debate in my mind whether or not there existed an antagonism between opium and stramonium; hence, I resolved, on the first opportunity that presented itself, to test them. But, having no chance to experiment, it looked to me as though the problem would never be solved, as far as I was concerned. Yet an unlooked for accident has more than rewarded my patience, and I can say with much pleasure and confidence that one is antagonistic to the other. I feel perfectly satisfied that in opium we have an antidote for belladonna and stramonium. Should cases of poisoning occur from either of those drugs, and the patient seen sufficiently early, there is no need of resorting to them. But, on the other hand, if the poison has been taken up by absorption, we then need an agent to hunt it up and undo what has been done.

To the case in question. Was called on the 1st of October, about half past one o'clock in the morning, to see a mother and her two daughters. They had had paroxysms of ague, and the mother resolved on being her own doctor. She conceived the idea that fennel seed was the medicine she wanted. She sent one of her little girls to a German drug store for the aforesaid fennel seed. Having obtained them, she at once proceeded to make a tea; the strength of it I can not tell. When I was called in, I found the mother and her two children were in a frightful condition. The mother and oldest girl were raving like maniacs, while the youngest girl was rapidly sinking into a state of coma. I soon perceived that they all were laboring under the influence of some powerful narcotic, but what one of them I was unable to determine. I called the attendant, and was informed that she had made a tea of fennel seed, and had drank freely of it

herself, and had given the children one or two cups full. I immediately inspected the vessel containing the tea, and to my great horror I found that my patient had been poisoned with stramonium. Two of them were maniacs, and the other almost expiring. What must be done? To think of giving emetics would be out of the question. Hence, I resolved that *Opium* should be their saviour. I sat down and wrote two prescriptions, one for two ounces of *Tincture opii*, and the other for *Morphia sulphatis*, gr. viii., in two ounces *Aqua*. Ordered the mother and girl to have one one teaspoonfull every half hour, and the younger child was treated with the solution mentioned above. The hypodermic syringe was called into use every half hour. I injected beneath the skin two drachms of the liquid. The child recovered rapidly, but the mother and the oldest girl seemed to defy treatment. What must I do? I could not give them up to die! About this time it was daylight. I thought that I would change the *Tincture opii* for *Morphia*. Accordingly I wrote for twelve grains of *Morphia sulphatis*, to be divided into four parts, one every two hours, and at the same time for

R. *Ammonia, Arom. Spts.*, 3 ij.

*Syr. Zingiberis*, 3 ij.

*Misce. fiat mist.*

*Sig.*—Two tablespoonfulls in a wine glass of water every half hour.

The ammonia seemed to give them more strength, but was far from quieting their ravings, and not until the second dose of *Morphia* was given, could I perceive any contraction of the pupils. At twelve o'clock, October 20th, the last dose of *Morphia* was given. Precisely at half past one the young lady fell off into a sleep; the mother soon followed. I ordered every thing to be kept quiet. I called again at five in the evening, but finding they were still asleep; did not disturb them. Called at nine; found them both sitting up in bed, but very weak. The youngest girl was drinking some tea, but not made from the seed of stramonium. The mother

and the other girl both refused to eat. Soon went to sleep again ; rested quietly all night, and in a few days they were attending to their labors as usual.

In presenting this to your readers, I do not claim any thing new, but there is one thing that I feel assured of: I hav determined the truth of it myself.

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### CASE OF POISONING BY FLUID EXTRACT OF GELSEMINUM.

BY W. F. HANI, OF MIDDLEBURY, ELKHART CO., INDIANA.

PROF. AILEN—*Kind Sir :*

As one of the Alumni of Rush Medical College, I would ask to transmit to the profession, through the columns of your JOURNAL, the report of a case of poisoning and death by the article in *Materia Medica* known as Gelsemium Semipervirens, or yellow jessamine.

J. F. came to my office on the evening of March 24th, 1868, and stated to me that his son F., aged eighteen months, had taken part of the contents of a vial of medicine which was given him to play with by his mother, and which had been prescribed for him about a month previously while laboring under an attack of acute pneumonia. After taking it he, in fifteen or twenty minutes, became so stupid that he could not be aroused by his mother. I then asked him how long since he had taken the dose, when he replied, about one-half hour since ; upon which he handed me the vial that contained the fatal draught. Upon examination I found it contained the *Fluid extract of gelseminum*. I then told him that F., as I thought, had taken a poisonous dose of the article. He then wanted to know where Dr. J. Yagerlenner was, who had prescribed the medicine. I told him the doctor had just gone to his home, and he had better see the doctor immediately and have him see the boy. When the doctor arrived, he found the child retching and vomit-

ing, and difficult to arouse. Dr. J. immediately gave him brandy, but only succeeded in getting him to swallow about a teaspoonful, when F. fell into profound stupor which lasted until life was extinct. About two hours subsequent to Dr. J.'s arrival I was sent for, and upon arriving and hearing the doctor's statement, I examined the case and found F. apparently lifeless, face and lips blue and livid, no pulse at the wrist nor at the carotids, pupils large and dilated, and upon examining the heart found the palpitation slow and very feeble, but continued to beat twenty minutes when life ceased. The amount of fluid extract taken by F., was, as stated by one of the parents, about thirty drops.

Now, as I am unable to find any authenticated cases of poisoning and death from the effects of yellow jessamine, I would be pleased to hear from some professional brother, through the columns of your JOURNAL, of any of the foregoing cases, and upon the best means and remedies adapted to counteract the poisonous effects of the yellow jessamine. My own opinion is inclined to be that emetics administered immediately with the stomach pump, and *Opium* given as an antidote, or some of the preparations of *Strychnia*. My reasons for resorting to *Opium* and *Strychnia* are based upon the fact that they produce contraction of the pupil—the direct opposite of the condition of the pupil in this case.

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#### LACERATION OF THE PERINEUM AND POST PARTUM HÆMORRHAGE.

*Two Cases reported to the Chicago Medical Society,*

BY HIRAM WANZER, M.D., CHICAGO, ILL.

CASE No. 1.—I was called to Mrs. B., aet. 28, August 13th last. She had been in labor with her first child twenty hours, under the care of a midwife. Upon digital examination, I found the head presenting at the superior strait, the os uteri

soft and moderately dilated, the soft parts were quite unrelaxed, and there was also rigidity of the perineum. The pains up to my arrival had been energetic, but the uterine contractions failed, even after complete dilation of the os, to increase the slightest advancement of the head. After waiting some four hours longer, I applied the forceps. The difficulties were somewhat increased by the mal-position of the head, the forehead looking to the left sacro-iliac junction, and the occiput to the right; consequently the blades were applied to the forehead and occiput of the child. It required a degree of force to dislodge the head, and bring it down into the excavation. During traction she complained severely of crampings. Had I withdrawn the instruments after the head's descent, and left her to the unassisted powers of nature, in order that the perineum and soft parts might relax physiologically, delivery might possibly have been accomplished without further interference, but I was fearful that further delay would not only increase the exhaustion of the mother, but compromise the life of the child. The rapid delivery of a preternaturally large head, apparently out of proportion to the parts of the mother, caused not only the complete division of the perineum, but also the laceration of both external and internal sphincter ani muscles, together with an involvement of the recto-vaginal septum, of one and a half inches, opening the vagina and rectum into one passage. A full anodyne being given, I immediately coapted the edges of the lacerated perineum by two deep ligatures. One of them came away on the 2nd, the other on the 5th day. Granulations at this time were quite luxuriant at the bottom of the wound. She was placed upon the side, and vaginal enemas of tepid waters were daily used, and at no time did the water escape by the rectum. The bowels were kept constipated by the *Tinct. Opii* for the first ten days, when an oleaginous enema was given, inducing a free evacuation without suffering. They were used occasionally for two weeks longer, while strict recumbency was enjoined. She was at this time able to resume her household duties. Her diet consisted of

fluid nourishment; such as could be absorbed into the blood with as little faecal residue as possible. The six weeks following parturition, she complained of want of power or contractility in the sphincters, to restrain the faeces, but since that time she states that she can hold them indefinitely; showing the ultimate possibility of their tonicity being re-established. Both the mother and child have done well. I would ask the experience of the society in laceration of the perineum, caused by the maladroit use of instruments, or otherwise, and their method of procedure.

CASE No. 2.—I was called Sunday, August 30th last, to Mrs. C., æt. 31, at  $7\frac{1}{2}$  A.M. She had been in labor with her seventh child since 2 o'clock that morning. She stated that the pains had been quite energetic up to the time of my arrival. Upon examination, the os uteri was fully dilated, the vertex presenting, the soft parts well relaxed, and in less than three-quarters of an hour the child was born. I waited a few moments, and proceeded to the delivery of the placenta. Upon gentle traction of the cord there was some haemorrhage, not sufficient to cause alarm; there had been none during labor. After the delivery of the after-birth, gentle frictions over the uterus were applied, until sufficient contractility of the organ was insured. The adjustment of a moderately tight bandage was not neglected. Upon leaving the patient, I requested them to summon me immediately should any thing occur unfavorably; otherwise I would call the next day. In about an hour a most alarming and exhausting haemorrhage supervened. The patient, in a few hours' time, had been reduced from full health and plethora, to the sunken eye, the palor, the collapsed and cold surface, and the almost pulseless condition we find in Asiatic cholera. The blood clots from the mouth of the uterus and vagina were immediately removed; the pillows taken from under the head; the foot of the bed was elevated to an angle of about thirty degrees. Pressure and friction were applied over the uterus until the organ was felt to contract sufficiently; also cold applications to the vulva, warmth to the feet, and re-adjust-

ment of the bandage. Two drachms of *Tinct. Ergot* was given every hour, with perseverance in *position*, and perfect quietude and recumbency. The haemorrhage, in a short time, was completely arrested, and she entered upon convalescence. She states she had suffered from post-partum haemorrhage at her three last accouchments, and in one instance had haemorrhage before labor, but never to an alarming extent. As there was no complication apparent in the case, I can not ascribe cause for the accident. Nothing abnormal had transpired during gestation or the delivery. I regard *position* our sheet-anchor in these alarming cases; but for it the mother might have perished. I view all other agents simply valuable auxiliaries in post-partum and in other exhausting haemorrhages.

No. 172 WEST TWELFTH STREET.

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### BOOK NOTICES.

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**OUTLINES OF PHYSIOLOGY, HUMAN AND COMPARATIVE.** By John Marshall, F.R.S., Professor of Surgery in the University College Hospital; with additions by Francis G. Smith, M.D., Professor of Institutes of Medicine in the University of Pennsylvania. Henry C. Lea, Philadelphia. 1868.

Time and space alike forbid the indulgence of the desire to give an extended notice of this excellent treatise on physiology. Still, however, we can not refrain from saying a few words in commendation of a work which we believe will become popular, among students at least, so soon as they shall have become familiar with some of its merits.

Unlike most book-makers, the author of this volume seems to have been troubled by no lack of subject-matter wherewith to make up a saleable article for the book market. His difficulties have been of quite another character, and the magnitude of his subject, and not less the abundant resources

at his command, have tempted him to exceed by far the limits of an unpretending treatise as originally designed, and give to the profession a work which is destined to take high rank amongst professional classics. The book is not the less attractive in that it presents considerable variation from the stereotyped plan of most of the physiological treatises which are at present accessible to the student and to the general reader, and some of these variations in the mode of arrangement and presentation of the subjects, while they may meet the criticism of the hypercritical, will, at the same time, secure the commendation of the tyro who will be grateful to the author who has given him so extensive a view of the science, upon his knowledge of which must mainly rest his hopes of attaining practical accuracy in his professional career. While, as has been already stated, the author has drawn largely upon the stores of physiological science accumulated by preceding and cotemporary observers, he has been by no means only a compiler, but gives evidence of laborious, patient, and intelligent investigation, and careful classification and generalization.

The work is abundantly illustrated with wood cuts, taken from the most reliable authorities, many of them being designed from the author's own observations, and are truthful and well executed. We are glad to perceive that the author arrays himself against the theory of spontaneous generation, which has recently, in France, received a new impetus from the fallacious experiments and sophistical arguments of Ponchet and others: the axiom "*omne vivum ex ovo*," is as clearly unassailable to-day as when it was proclaimed by the great father of medicine two thousand years ago. Of reproduction the author has assumed the division of Carpenter and the older physiologists, into two kinds, sexual and non-sexual. This decision is certainly based upon incomplete analysis and partial comparison of the phases of the reproductive process in different organisms, and must eventually be abandoned when comparisons have been made between these processes in their totalities. Germination in the plant,

or fissiparous or gemmiparous reproduction in the lower animals, has apparently little in common with the fertilization of the ovum in the human organism, and if these two phenomena should be regarded as the corresponding phases in the reproductive processes of the classes of organisms referred to, the processes might with propriety, indeed ought to be placed in different categories. But when the phenomena of gemmation or effission respectively are compared with those accompanying the phase of placental separation in the reproductive process of the higher mammalia, the identity of phenomena is so striking as to necessitate the conclusion that these are corresponding phases, and that other correspondencies will be recognized when a more accurate parallelism shall be maintained in the comparison of reproductive processes throughout the entire organic world.

We would here take exception to the total silence maintained by our author, in common with nearly every other physiologist regarding the influence of moral and physical agencies in the modification of physical function and structure. Man is considered exclusively as a material organism, subject to the operations of material agencies, either external or internal, solely, or at the furthest, to those of a purely emotional character. In this there is an apparent incompleteness in the analysis of functional correlations; for, while the most careful and laborious research has been expended in the investigation of, and due consideration given to the relations of the lower portions of the nervous system with the purely vegetative and animal functions of the organism, and to a certain extent those of the emotional centre, the mesocerephale with the same have also been considered. The still more important relations of this latter portion of the nervous system, with its dominant organ, the cerebral hemispheres, have been tacitly abandoned by the physician to the metaphysician as some thing truly *μετα τα φυσικα*, and those of the organ of intellection and volition, with all the rest of the organism over which it is theoretically admitted to dominate, is totally ignored.' This disregard of the psychical

relations of the human organism must, by excluding the flood of light which might be thrown thence upon the more subtle and intricate modifications of function and structure, both in health and disease, which now so completely baffle the investigator, must prove an effectual barrier to the progress, not only of physiological science, but of therapeutical efficiency.

We demand for physiology a more extensive scope, a more comprehensive insight into man's nature, inseparably three-fold, and insist that in restricting his investigations to the purely physical and material aspects of the subject, and the abandonment of the moral and intellectual to the theologian and the metaphysician exclusively, the physiologist has relinquished the richest portions of his rightful domain.

Of the typographical and mechanical work it is superfluous to say any thing more in commendation than that it comes from the press of H. C. Lea, of Philadelphia, whose name alone is a sufficient guarantee of excellence in this department.

W. H.

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RECHERCHES' experimental sur une fonction du foie consistant dans la séparation de la cholestérine du sang et son élimination sous forme de stercoreine (SeroLINE de Boudet) par Austin Flint, fils, Doctor en Médecine, Professeur de physiologie et de microscopie au College de Médecine de Bellevue Hôpital à New York, et au College de Long Island, Hospital à Brooklyn; membre de l' academie de Médecine de New York, etc. Paris: Germer Ballière, Libraire Editeur. New York: D. Appleton & Co., 1868.

We are indebted to the courtesy of Messrs. Appleton & Co. for the valuable treatise whose title is prefixed, a familiar friend, not the less welcome that it comes to us clothed in a foreign garb. The appearance of this work in its new guise is a double source of congratulation; first, to its author, that he has received so flattering a recognition, which he so richly merits, in what he himself terms "*le foyer de la science physiologique*," and secondly, to that "focus" itself in that it has acquired an additional ray of scientific light to aid in

maintaining its perennial brightness. It is not inappropriate too, that our young American physiologist should, in this French edition of his work, make to the physiology of France a testimonial of acknowledgment of the source from which he undoubtedly derived his scientific facts, not less than his inspiration to the labor of investigation to which we are indebted for such solid results.

#### FLINT ON CHOLESTERINE.

The author has commenced his investigation into the subject cholesterine, *ab initio*, by a thorough analysis of the literature of the subject, from its discovery by Pouletier de la Salle, in 1782, to the present day, starting with the justifiable assumption of Robin et Werdeil, that the physiological role which it fills in the economy, is unknown; he is led to the hypothesis of its analogy, both physiological and pathological, with urea; maintaining that the discovery of cholesterine will effect as much toward the elucidation of many obscure maladies which may be hereafter classified under the title cholesteræmia, as the discovery of urea has effected for those now recognized under the title of uræmia.

The author's experiments upon bile, performed upon dogs, furnish an interesting explanation of the discrepancies perceptible between those of Bidder and Schmidt and Schwan, and those of Blondlot, and at the same time establish the possibility of the restoration of continuity, once interrupted, in the excretory ducts of glands, and likewise determine a new function for the bile, viz.: the separation of cholesterine from the blood. In pursuing his investigations further, he traces this excrementitious substance, cholesterine, from its formation by the disintegration of tissue and its transportation in the current of the circulation to its separation from the blood by means of the liver, and also the disturbances produced in the economy by its retention in the blood, in consequence of failure in the excretory activity of the liver, which condition he very appropriately designates cholesteræmia. Regarding the physiological relations of cholesterine and

stercorine, the author has demonstrated the four following propositions :

1st. Cholesterine is an excremential matter, produced by the disassimilation of nerve substance, and absorbed by the blood.

2nd. It is separated from the blood, during its passage through the liver, enters into the composition of the bile, to which it gives its excrementitious character.

3rd. It is poured out with the bile into the upper portion of the small intestine, where the digestive process changes it into stercorine, under which form it is enveloped in the faeces.\*

Apropos of the pathological relations of these two bodies, our author concludes,

1st. That the proportion of cholesterine in the blood is enormously increased, which shows that an organic change in the liver has prevented its separation from this fluid.

2nd. A corresponding diminution of stercorine in the faeces shows that the cholesterine is not poured into the alimentary canal in its normal quantity.

4th. Stercorine, which constitutes the great excrementitious element in the faeces, is one of the most important excrements resulting from the usage of the economy.

It is impossible, within the narrow limits at our command for the purpose, to do full justice to Dr. Flint's monograph ; it must be read, yes, studied, to be appreciated, and should be studied, and carefully studied, by every one pretending to physiological knowledge, or practical excellence.

W. H.

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We have received the third number, for October, 1868, of the *Revista Medico Quinorgica y Dentistica. Periódico de las Ciencias Médicas En Todas Sus Ramos, Y. Eco De La*

\* The discovery, in February, 1868, by the writer, of cholesterine in the faeces of a patient in whom disintegration of the mucous surface of the duodenum had been already established by concomitant symptoms, furnishes confirmation of the proposition.

W. H.

Brensa, Médico Estranjira. Bedactores Y. Boprntarius, Los Sres : Wilson Y. Gonzales, Habana, Cuba.

We are most happy to extend a hearty welcome to this youthful stranger from the Queen of the Antilles, and to accede to the request of its editors "to be placed upon our exchange list." Under any circumstances we should receive this journal with pleasure upon its own merits solely, but our pleasure is enhanced by so substantial evidence of the status and *esprit de corps* of our professional brethren in the West Indies. "*La Revista*" is a quarterly of upwards of one hundred pages per number, containing several original articles of a high order of merit, and also admirable selections from the best American and European journals. Many of the articles are handsomely illustrated, and altogether the journal presents an appearance highly creditable to its editors and its publishers.

We shall take the liberty, from time to time, to translate from its pages such items as may appear likely to interest our own readers, as "a fair exchange is no robbery." W. H.

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ALBUMINURIA: Translated from *La Revista Medico Quirurgica*, Habana.

Professor Semola, of Naples, announces the opinion that the passage of albumen into the urine in the disease called "Bright's," is a necessary consequence of a general deficiency of nutrition, in virtue of which the albumen, becoming incapable of exercising its functions, is eliminated by the kidneys as a substance foreign to the organism. According to this theory, the alteration of these organs occupies only a secondary part in the pathology of the disease; and yet the condition of the kidneys is an important *datum* in the prognosis. Professor Semola protests against the ideas of those who pretend to explain or to solve the question by virtue of mere anatomical deductions. One of the diagnostic signs which distinguish organic from symptomatic albuminuria, rests upon the quantity of urea, which in true Bright's disease, diminishes from the first appearance of the albumen, and at

a later period accumulates in the blood. The same occurs with the sulphates. Of artificial (?) albuminurias, that which originates from suppression of the cutaneous functions, most closely assimilates to Bright's disease. This suppression impedes, on the one hand, the oxidation of substances introduced into the system under the form of peptones, (products of the digestion of albumen), and, on the other hand, occasions congestion of the viscera, especially of the kidneys. Thus, according to this author, Bright's disease is not the result of a primitive anatomical lesion, but the result of that double series of effects which succeeds more or less quickly the suppression of the function of the skin. Consequently, the efforts of the physician ought to be directed to the re-establishment of these functions, by augmenting the oxydation of the peptones and effecting the disappearance of the renal congestion. Amongst the remedies best adapted to this object are to be reckoned ordinary sudorifics, or in obstinate cases, bottles of hot air,\* followed always by cool or cold shower baths, the preparations of arsenic, and inhalations of oxygen. The diet ought to be feculent or vegetable, with very little meat.

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RESULTS OF SURGICAL OPERATIONS IN PORTUGAL.—In examining the work of Dr. Antonio Maria Barbosa, published in *la Gaceta Médica de Lisboa*, we find that the results of special operations performed by Portugese and French surgeons in Paris and Lisbon, are less favorable than those obtained by the surgeons of London. In the operations for hernia and lithotomy by Dr. Barbosa, we perceive that the advantage is clearly on the side of the English surgeons. Among the former the mortality was as follows: Hospitals of Paris, 60.45 per cent.; in Lisbon, 58.82 per cent.; in London, 50.75 per cent. Among lithotomy operations, the mortality was 37.3 per cent.; in the hospitals of Paris, 35.7 per cent.; and 21.5 per cent. in London.—*Gazeta Médica de Bahia*.

\* The Professor does not state how the "bottles of hot air" must be applied or used.

**PROF. TURK.** — The heirs of this distinguished physician have donated his library, comprising more than a thousand volumes, amongst which are many works of great merit, to the Royal Society of Physicians of Vienna. The instruments for laryngoscopy were assigned to the Surgical Clinic of Professor Dumrischen, and the duplicates to the department of the hospital which was in the charge of the deceased.—*Allgemeine Wiener Medical Zeitung.*

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**PROFESSOR F. VERDUGO** died recently in Salamanca, Spain, at the advanced age of 105 years, after having practiced medicine during eighty years.—*Revista Medico Quirurgica, Habana.*

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### PHILADELPHIA CORRESPONDENCE.

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PHILADELPHIA, November 12, 1868.

**EDITOR CHICAGO MEDICAL JOURNAL:** As I mentioned in my last communication to your columns, as probably to take place, the reception to Professors Gross and Pancoast came off at the Foyer of the Academy of Music on Saturday evening, October 25, and it was a magnificent affair. Many of the distinguished medical gentlemen were present from other cities, and it was a brilliant entertainment, well conducted, and eminently well merited.

I shall close my short series of letters on the subject of uterine diseases with this letter, noticing in the first place the *hypertrophied uterus*. It is impossible to find an ulcerated, or indeed even an inflamed uterus, without hypertrophy more or less marked. But there are cases of enlargement without any present ulceration or inflammation even. I have noted sixty-five cases of hypertrophied uteri with no other form of disease existing, and as a natural sequence, more or less prolapse in all. The womb increasing in all its dimensions, necessarily

increases its weight. This augmentation of weight forces the uteris down into the vagina, notwithstanding the vaginal walls may be quite tense, and the patient presents herself for treatment for "falling of the womb." How shall we treat this? By pessaries? By caustics? Better than pessaries, but better than all, by sponge tents. They *may* fail, but in the sixty-five cases before mentioned, in forty-one the sponge tents were all the treatment. Tent after tent was introduced, and in thirty-seven the cure was complete. So far as our observation extends, the larger proportion of prolapsed uteri are due to hypertrophy, and when this is cured the prolapse is cured.

In hypertrophied and in inflamed and abraded uteri, the knife has been recommended by Louis especially, and he advised the cervix in many cases — especially in occluded os uteri — to be incised. In our clinic, and in my own private practice, the mucous membrane lining the cavity of cervix, as well as the mucous covering of the os, has been freely scarified by bistoury, with the most astonishingly rapid improvement. Incisions — several in number — are made in all directions, remembering the great thickness of cervical mucous membrane, and blood allowed to freely exude. Oftentimes an hypertrophy will melt away, and congestion disappear before the patient rises from the bed. I would strongly urge it as a plan worthy of very general use. As I have before remarked, the pathology, as well as treatment, of uterine diseases, are rapidly progressing. It is a specialty demanding the attention and study of the general practitioner of the country, for to him will fall a large amount of uterine diseases, which, if properly managed, will be the best tests and proofs of the value of any suggestion I may have dropped in these letters.

If I have aided one mind upon this subject, through the large circulation of as valuable journal as yours, I shall be well satisfied.

I have an interesting case of a large uterine polypus to present you with in my next. Yours, etc.,

E. R. H.

## EDITORIAL.

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### *Publisher's Notice.*

It is peculiarly gratifying to the conductors of a publication, with many weights and responsibilities to sustain, that the outside reader can know nothing of, to receive assurances of favor and esteem from his patrons. From the time of its commencement up to the beginning of the present year, **THE JOURNAL** was conducted by professional men whose one aim was to supply the want for such a periodical which the rapid growth of the profession had created. The attention of these was entirely absorbed in furnishing the most valuable articles and intelligence; so that it naturally happened that the business of the periodical, which is no less important and necessary to its existence than the editorial management, fell into neglect. This will account for the mistakes which required no inconsiderable amount of labor and trouble on the part of the present conductors to correct. Efforts have been made during the present year to secure the payment of the subscriptions on **THE JOURNAL**, many of which had been accumulating, and which amounted in the aggregate to a large sum. When, therefore, the bills were presented to the delinquent subscribers, some of them, having been long accustomed to receive **THE JOURNAL** for nothing, grumbled excessively, and request a second or third presentation of their account. The replies of these discontented spirits go to make up the sum of troubles which the editor is obliged to experience; but it was at least satisfactory to learn that the majority of them with the honorable spirit which ever characterizes the worthy members of the profession, did not repudiate so just a debt. It has been gratifying too, not a little, to receive remittances accompanied with an expression of appreciation of **THE JOURNAL** and of a determination to render prompt payment in future. The following characteristic letter was received while the last number was in press:

November 10, 1868.

*To the Editor of THE JOURNAL:*

Your bill of November 1st is received. Enclosed find post-office order for \$10, for which please receipt and return. I thank you for your indulgence. The times have been hard with me, or I should have remitted sooner. The offer of discount is a kind one upon your part, but I consider that **THE JOURNAL** is worth its price, and I send you the full amount. Continue sending it to me. I can't do without it. It is the best I have seen.

Very respectfully.

Hereafter, all letters and communications to the Editor of **THE JOURNAL** will be addressed, **CHICAGO MEDICAL JOURNAL**, No. 71 Dearborn Street Chicago, Ill. The box at the post office has been discontinued.

***News and Items.***

Dr. Lockhart having resigned the Superintendency of the Indiana Insane Asylum, the Board of Trustees have elected Dr. Orpheus Everts, of Michigan City, to the position. An appointment eminently "fit to be made."

S. E. SCANLAND, M.D., a well known graduate of "Rush," has drawn a first-class prize in the great lottery of life, having captured and led to the hymeneal altar, the beautiful and accomplished Miss Agnes Leonard, well known to the literary world as "Mollie Myrtle." Rev. D. P. Henderson officiated. The very best wishes of THE JOURNAL attend them.

The entertainment given by the profession and citizens of Philadelphia, to Professors Gross and Pancoast, on their return from Europe, proved an eminent success, just as it should have been. We shall give an extract from the beautiful address of Prof. Gross in the next number.

The latest editions of Flint's Practice of Medicine, etc.; the second volume of Aitken's Practice, are respectively on file for speedy notice.

The New York Hospital is to be torn down, and the grounds leased for \$140,000 annually. The Hospital will be reerected at Bloomingdale.

The policy is announced that hereafter surgical and medical appointments in the army, navy, and civil service of the government will be made only as subservient to political considerations. The notice seems to be a work of sepererogation.

W. ALPHONSO Wood, M.D., a highly respected practitioner of medicine, is reported to have died recently and suddenly. The notice sent to the JOURNAL is authenticated by no name or date. When they are sent us we shall publish the resolutions said to have been passed by a local society on the occasion. Dr. Wood was one of nature's noblemen, still further ennobled by professional and civic culture.

**Books Received.**

Transactions of the American Medical Association, Instituted 1847., Vol. XIX, Philadelphia; printed for the Association. Collins, printer, 705 Jayne Street, 1868 Pp. Notice next month.

THE TWELFTH EDITION OF ELLIS'S MEDICAL FORMULARY, heretofore frequently mentioned in THE JOURNAL, has come to hand. Pp. 374. W. B. Keen & Co., Chicago.

This is really a *new* edition of a well-known work. Many formulæ of little value have been omitted, the table of doses has been carefully corrected, and new formulæ of value have been introduced. The new classes of anti-emetics and disinfectants, and also the use of atomized fluids receive careful attention. A new feature is introduced in a full index of diseases, with references to the members of principal ingredients of the formulæ applicable to each.

A HAND BOOK OF VACCINATION, by Edward C. Seaton, M.D., Medical Instructor to the Privy Council. Philadelphia: J. B. Lippincott & Co., 1868. Pp. 383.

A complete *resume* of what is known on the subject. We shall refer to it hereafter.

A TREATISE ON PHYSIOLOGY AND HYGIENE, for Schools, Families and Colleges. By J. C. Dalton, M.D., Prof. of Physiology in the College of Physicians and Surgeons, New York. With Illustrations. New York: Harper & Brothers, Publishers. London: Sampson Low, Son & Marston. 1868. Pp. 399.

The professional reputation of the author of this book is fully sustained in this most dangerous of all books to write. It is every way reliable, compact, clear, fully adapted to the purpose set forth in its title.

THE OPIUM HABIT. With Suggestions as to the Remedy. New York: Harper & Brothers, Publishers, Franklin Square. 1868. Pp. 235. Notice next No.

In the next number also we shall mention our exchanges, particularly those non-professional. We regret the paucity of

our pages, and only regret that we had not double the room to contain the *crowd* of good things that are now pressing upon our pages. An immensity has to go over to subsequent issues.

**CONSTIPATED BOWELS: The Various Causes, and the Different Means of Cure.** By S. B. BIRCH, M.D., Member of the Royal College of Physicians of London, etc., etc., etc. From the third London edition. Pp. 181. Philadelphia: Lindsay & Blakiston. 1868. Price, \$1.25, at Keen & Co.'s

A very creditable monograph, and well worth perusal. The author is evidently a man of practical common sense. The name of the publishers is sufficient guaranty for the style of the publication.

#### PROCEEDINGS OF THE CHICAGO MEDICAL SOCIETY.

DURING the past few months, the meetings of this Society have been held but once in four weeks. Hereafter they will be held weekly. The following are instructive cases, with the report of which pathological specimens were presented:

##### REMOVAL OF A LARGE OVARIAN TUMOR — RECOVERY.

Prof. Powell presented an unusually large ovarian tumor, which he had removed from a patient, thirty years of age. The patient first observed a slight swelling of the abdomen, about a year ago, the growth of which, however, did not cause her special inconvenience till January last. From this time to the day of the operation, August 31st, the abdomen became very rapidly distended. Paracentesis had quite recently been twice performed by other physicians, with the discharge of very little fluid. The tumor was removed through an opening in the abdomen, extending from the ensiform cartilage to the symphysis pubis. Before the tumor could be brought through the opening, it was necessary to evacuate large quantities of fluid from several cysts, and to break up with the hand several extensive adhesions. After the peduncle was firmly tied with a strong linen cord, it was divided and returned within the abdomen. The external wound was closed by means of twisted sutures, and supported by a flannel bandage. The tumor, with the serum contained in its cysts, weighed sixty pounds. The patient made a rapid recovery with scarcely any marked excitement of the circulation, diminution of appetite, or loss of sleep.

##### CANCER OF THE STOMACH.

Dr. Bogue exhibited the stomach of a male patient, 69 years of age, who had never, with scarcely any exception, suffered from sickness, till a year before his death. He first experienced slight symptoms of dyspepsia. There was gradual loss of appetite, with emaciation. Four months previous to death, there was first observed a small tumor, with tenderness, in the region of the stomach. The symptoms increased in severity, although at no time was there much acute pain or much vomiting. On the tenth of July he was wholly unable to swallow. From this time to the day of his

death, August 29th, he was nourished by nutritive enemata. At the autopsy, the stomach was found very much thickened and contracted, with a very narrow passage through it, and almost no trace of mucous membrane. The other organs of the body were from a similar disease.

#### CONCRETION FROM THE FEMALE URETHRA.

Prof. Miller exhibited a small concretion, one-third of an inch long, and one-half an inch in circumference, which he had found impacted in the urethra of a female twenty-five years of age. It was peculiarly grooved in such a manner that the urine could slowly escape through it, even when the mucous membrane was constricted around it. The case was interesting from the fact that it had been under the care of three different physicians, who had treated it as inflammation of the bladder, or as uterine disease.

#### RAPID INFILTRATION OF A LUNG WITH TUBERCULOUS MATTER.

Prof. Ross exhibited the left lung of a young man who died at the Cook County Hospital. The case was most remarkable from the fact that the patient had been strong and rugged till eight weeks before death. He died two weeks after entering the hospital, with the characteristic emaciation, hectic night sweats, and expectoration. There was almost no respiratory motion on the left side. The left lung was found almost completely solidified with tuberculous deposit. There were large cavities at the apex.

Prof. Ross also presented a typical specimen of "hob-nail liver." The organ was covered with the characteristic nodules, and weighed but one and three-fourth pounds. The patient, a female aged twenty-five years, had been of intemperate habits. Death occurred two weeks after the patient entered the hospital, with all the symptoms of typhoid fever. The characteristic lesions of this disease were found in the ilium.

#### SARCOMA OF THE CHOROID.

Dr. Holmes exhibited an eye which he had recently removed from a patient at the Chicago Charitable Eye and Ear Infirmary. There was a well defined tumor occupying the external and lower portion of the globe, and extending from very near the ciliary muscle to the optic nerve. At the first examination, three weeks previous to the extirpation, the tumor could be readily seen by concentrated light, and still better with the ophthalmoscope. There was no perception of light except at the inner and upper portion of the retina. The pupil, of normal size, acted quite freely under the influence of light. There had been no pain. The globe was not hard on palpation. The cornea had not lost its sensitiveness, although an increase of the intraocular tension and loss of sensibility of the cornea are very frequent symptoms of choroidal tumor. There was no trembling of the retina, which is usually observed when there is detachment of the retina. It was nearly a year since the patient first observed any impairment of vision. Two days before entering the Infirmary, the patient presented himself, suffering excruciating pain in and around the eye, with much redness and edema of the conjunctiva and hemorrhage behind the lens. These symptoms had appeared very suddenly. A horizontal section of the globe after its removal revealed a sarcomatous tumor of the choroid, situated as above described. The central portions consisted of a soft, pulpy, yellowish red mass, surrounded by a layer of black tissue, nearly a line in thickness. Dr. Lyman made a microscopic examination of the specimen, and states that the tumor is sarcomatous, with characteristics as described on page 460 of the translation of Stellwag. The outer portion of the tumor is dark from the presence of black pigment in many of the cells.